

A Fire in Schermerhorn Extension

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This is to report upon a fire, or certain features of one, that occurred within a period roughly bounded by the years 1935 and 1965, with its center mainly at Columbia University, in New York City. The origin of this conflagration is uncertain. There is room for several theories. One view holds that it was caused by sparks from a nearby bonfire in Upper New York State. Another holds that it was due to an incendiary bomb sent through the mails at some time in October 1938. (The letters B and O are clearly to be seen on some fragments of a book container that was later found in the debris.) Either viewpoint leads us to an infinite regress. I support them both.

The damage done is also difficult to assess. The blaze was never fully contained and is known to have spread for many miles in all directions. It may still be smoldering somewhere. Some have argued that it didn't do much harm—that the buildings burned had already been condemned and were a liability for everyone. A few have said that it had but little effect, if any, upon the older structures, but I think that such folks must be blind.

My report upon the fire will be a partial one, in two respects. I cannot cover every aspect of it or all its stages of development; this would take too long. Also, it will exaggerate the role of certain heroes or villains who attempted to extinguish or extend the blaze. It is difficult, even for a skilled observer like myself, to be entirely without bias. In presenting my analysis, I shall rely on certain documents for support—exhibits, so to speak, although they won't be visual in their nature.

This manuscript is an edited version of a talk delivered to the Psychology Colloquium at West Virginia University, April 14, 1986. Reprint requests should be sent to the author at 820 Emory Drive, Chapel Hill, NC 27514.

Exhibit 1

Here are some excerpts from a letter that I wrote to B. F. Skinner, Ph.D., on January 17th, 1935, from Hamilton, N.Y., the site of Colgate University:

I'm in bed with a cold and this may be brief. My main reason for writing just now is this: will you, by chance or design, be in this vicinity on the first Friday after Easter; and would you care to be the guest speaker at the Upper New York Psychologists' meeting at that time? I can't offer you a nickel . . . so there will be little more than the prestige or contact factor involved, and you can guess what that amounts to—Cornell, Syracuse, Buffalo, Hobart, Rochester, Albany, etc., will be represented. There will be some publicity attached—leave that to Mr. Dexter Tead, coiner of the "Red Raiders of the Chenamgo" . . . and you will be on a program with an unveiling ceremony . . . a portrait of "Peppy" Reed, old-time psychologist at Colgate, with a speech by that ex-President of Cornell . . . whose name I can't tell you without getting out of bed, but who has much news value . . .

There are other possibilities for a speaker on the program, but my aim is to keep the standard at least as high as Hull made it at Rochester last spring! You could talk about anything that you wished . . .

Hope things are going well at Harvard. How about one of those soundproof boxes of yours with all the gadgets? Can Gerbrands do the job at a reasonable figure?

My letter left Hamilton on the 17th and reached Cambridge on the 18th, when my friend immediately replied. This was before we had the benefit of sending mail by air. His reply is as follows.

Exhibit 2

I don't see why I shouldn't accept your invitation (with real thanks, old chap) for the 26th of April Would a 50-50 mixture of theory (very general and simple) and facts (nice pictures of curves) go well? . . .

Gerbrands and I are right now working out a standard model. All the fixings for less than \$50—or he doesn't get the job. You'll need a slow kymograph, too—say \$20 This would last for years and I would practically guarantee results.

My colleague came to Hamilton as promised, and gave his talk. He described his new experimental method and offered us

a "unitary formulation of several kinds of learning." He did a splendid job, dressed in his tuxedo like the others at the speaker's table. Also he brought with him the Box that he had mentioned—a \$45.00 bargain. With the help of a friend in the Physics Department, I got a cumulative recorder for less than half that price. I am sorry that I do not have a picture of the apparatus, but some of you may know what it was like.

Within the next two years, I used the Box in several experiments, with undergraduate collaboration. The first of these was suggested by my friend and found its way into a book that he was writing; the second dealt with the effects of different amounts of fasting upon the rate of lever pressing; and the third was one in which my rats were free to eat at any time of day or night, with a pellet of food for every lever press. Students stood watch by turns throughout the 24-hour period to monitor the operation and keep the food dispenser supplied with pellets.

Exhibit 3

This was written by Edward Menasian, class of '39, in a theme for English Composition, entitled "Embryo Psychologist."

Come on, rat, come on! Start poundin' will ya? I hope I don't fall asleep—swell business—Doc' walks in and finds great research psychologist asleep! This is a scream, though—put a muggy little rat in a food box, let him slave away by pounding on a lever for his food and we publish the curve as a finding in animal psychology . . . What a find! What's the matter, rat, not hungry? Well, hit the lever, you fool . . . Oh, well, I've only got a half hour more—wish I could get a curve out of him, though—Make Doc' feel pretty good. But what the heck, I ain't gonna eat for the rat—What good is it? What's the use, anyhow? Who's gonna run a fever looking at the eating chart of a white rat? . . . Hell, I wish I was back in bed.

Edward never got beyond the embryonic stage.

Exhibit 4

AN OPENING IN PSYCHOLOGY COLUMBIA COLLEGE. STAFF WOULD LIKE TO DISCUSS MATTER WITH YOU IN A VERY GENERAL WAY. CAN YOU LUNCH WITH US TOMOR-

ROW FRIDAY MEET ME 420 SCHERMERHORN EXTENSION AS NEAR TWELVE AS POSSIBLE TRAVEL PAID PLEASE WIRE COLLECT VIA WESTERN UNION.

A. T. POFFENBERGER,
COLUMBIA UNIVERSITY

This message was received in Hamilton at 3:12 P.M. on the 26th of May, and reached me moments after that. As soon as I regained by senses, I arranged for absence from my Friday classes, telegraphed Professor Poffenberger, and caught the morning train from Utica to New York. A scenic trip along the Mohawk and the Hudson brought me to the Chairman's office at the appointed hour. But that was 50 years ago, well before the miracle of 727's in the friendly skies. Now the journey takes much longer and costs a great deal more.

One week later I received a telegram from Gardner Murphy, who was the Department's representative in Columbia College. This is my . . .

Exhibit 5

DEFINITELY OFFER YOU INSTRUCTORSHIP THREE THOUSAND PLUS FIVE HUNDRED EXTENSION WOULD APPRECIATE REPLY AS SOON AS CONVENIENT.

Five days later, I accepted Gardner's offer and, a few days after that, one from Poffenberger, with the administration's blessing. On Labor Day of 1938, I drove my wife and daughter to New York. Our furnishings, together with the Box, were also sent that day, in a pea-picker's open truck. The Box was left in our 118th Street apartment for awhile, since there seemed to be no place for it in Schermerhorn Extension.

My colleagues at Columbia were impressive. A. T. Poffenberger was the expert in applied psychology; H. E. Garrett was our specialist in statistics; R. S. Woodworth, at the age of 69, was the unofficial Dean of American Psychology, revered by everyone; Carney Landis, known best for his studies in human emotion, was at the Psychiatric Institute on 168th Street; H. L. Hollingworth was the Barnard College Chairman, with Richard Youtz and Shirley Spragg as

newcomers on his staff; and, across 120th Street at Teachers College, there was E. L. Thorndike, better known, perhaps, than any of the rest. Gardner Murphy, under whom I was to work, along with Otto Klineberg and John Volkmann, was well known as a social psychologist and historian.

Finally, there was Carl John Warden, Columbia's animal behaviorist and the reigning figure on the second floor of Schermerhorn Extension, with whom I hoped I might find common cause. But when I told him of my interest in rat experimentation, he was quite unfriendly, saying that his graduate students needed all of the space available for research.

Schermerhorn Extension, in 1938, was not a place where you might expect a fire to break out, even with a Box as kindling. But now comes my next exhibit, from a letter written in October to my friend, who had taken an instructorship at the University of Minnesota, with a salary of \$1,960.00—much less than mine.

Exhibit 6

The book is great! . . . It comes up to, and goes beyond, everything I had anticipated, and I had Great Expectations. In my humble opinion, it is the most important single contribution that this century has seen in the field of psychology. As a beautiful example of inductive method and operationalism, it puts to shame the Hullites, the Titchenerians, et alia, with their high-powered deductions, their narrow applications, their physiologizing, and their vague dreamery of psychology-as-science. Hell! You know what I mean without all this big talk.

Later in that letter, I reported on my new position:

My courses are well under way [a graduate course in Perception, an undergraduate course in Advanced General, and discussion sections in Gardner Murphy's Introductory course within the College]. I am making all the proper contacts and I am getting quite an education. There is only one fly in the o, C. J. Warden. Everyone cautions me not to step on his toes or expect to do much animal work in the laboratory and for God's sake not to plan to give any problems to the Ph.D. candidates.

My search for working space was finally rewarded when Gardner Murphy gave me some of his. Two doors from his office, at the other end of the hall from

C.J.'s fortress, was a laboratory room that had been used in a study of "extra-sensory perception." It was exactly what I wanted; it even had a sound-resistant room within it, large enough for my Box and experimental subjects, animal or human. I removed all evidence of E.S.P. and, by 1940, I had demonstrated beyond the peradventure of a doubt, that albino rats would press a lever to turn off a light. I wrote my friend about this "darkness drive" on February 4th.

Exhibit 7

I have collected lots of data, under pretty crude conditions, and I'll have some improved apparatus before long. There are some puzzling things about the experiment and some very exciting things. I wish to God I could get rid of my teaching for this year and go at the thing in a big way—that is, spend all my time on it. Then I would have Warden at my throat!

Speaking of C.J. He has an assistant named [George] Klein who decided he would like to do a thesis with the Skinner apparatus, if he could get his boss's O.K. He caught C.J. in what was apparently a friendly mood and suggested, by way of approach, that he would be willing to construct a Skinner box for [Warden's] lab course. Warden went up in the air sky high! Red with rage and incoherent, he yelled at poor Klein: 'What do we want with one of those God-damned dirty little boxes—we've got plenty of apparatus, and better, in the lab now!'

I might have tried to do more work on light aversion at that time, if it hadn't been for World War II. I got involved in military matters, as did my friend in Minnesota. Also an important change took place in our Department. Murphy, who had been an assistant professor for 19 productive years, finally gave up waiting for a promotion. After much consideration and an attempt by the Department to retain him, he accepted a full professorship at City College, the chairmanship in Psychology, and a \$3,000.00 raise in pay. I was named as College Representative and given Gardner's office on the second floor. John Volkmann and I were asked to share the burden of the Introductory College course between us, with the discussion sections replaced by Friday demonstrations. Otto Klineberg, formerly a section man with John and me, was freed from his connection with the course.

By 1945, the war was over. John and I were back at Columbia, with our new assignment, while Nat Schoenfeld and I were hatching schemes for spreading reinforcement theory within the University and elsewhere. An idea of our collaboration can be taken from another letter to my friend, who had just been lured from Minnesota to Chair the Department of Psychology at the University of Indiana.

Exhibit 8

About the book. I've had it in mind for quite a while, as you know—a Skinner for Beginners. It became more and more apparent that something had to be done about the General course; Schoenfeld alone of those hereabouts really understood the viewpoint and was cooperative and critical enough to help me undertake the job; and finally it came to a point where *something* had to be started. Last summer I tossed the texts out of the window and started out on my own, with very encouraging results . . . I gave 'em eight pages of outline, and some reading assignments. Nat and I cooperate as follows: I work up the outline, he criticizes it, we talk it over, I rewrite. At all odd moments, at meals, etc., we talk about the next step. At the moment, with our second outline, I am doing the heavy lifting, but he will be playing more and more of a part . . .

By 1946, we had prepared a 37-page mimeographed Outline of our course, containing 214 paragraphs, to cover a year of work. By 1947, the Outline had become a mimeographed textbook, which we sold to students until it was supplanted by one of standard form—*Principles of Psychology*.

The next development is related to the book. [I'm still in Exhibit 8.] I am trying to put over a new deal in General here, beginning next year. Essentially, this is it: a one-year, eight-point course, two lectures weekly, and four hours of lab. I have the go-ahead sign from Garrett and I think I can get the College to give us Science credit. Volkman is deputized to construct the lab or see to its construction, to the tune of about 5,000 dollars as initial outlay. Experiments will parallel the text and lectures and will be few in number, say 8–10 major experiments. Rats will be our subjects, the slant being “biological,” and 15 or 20 all-purpose Skinner boxes will constitute our first standardized equipment. Every basic principle of the course will be demonstrated by actual experiment. Students will probably work in pairs under close supervision (two full-time assistants and a stockroom man will probably be required) . . . No apology, incidentally,

will be made for using rats as subjects; human experiments can come into the next course . . . The boys will never take an attitude test, calculate a threshold, map a touch spot or a color zone, etc., etc., but they will get their *general* principles. Just to show you how crazy I am, I'll tell you what I have in mind beyond the General . . . Later, and it may be much later, an experimental course in Discrimination, another in Conditioning, and another in Motivation, each ultimately with its own laboratory space and assistants. I'm going cautiously and avoiding toe-treading at present, but I'm ultimately going to *force* a good course in Social (or Verbal) Behavior and Abnormal; and I may even see the Graduate offerings . . . become Graduate in content as well as name.

These were strong words to be writing in 1945, but important changes did take place within the year that followed, mainly through the ministrations of John Volkman and Nat Schoenfeld. A first-course laboratory was constructed and equipped across the corridor from my office on the second floor, complete with Boxes of a new design and devices for recording rat behavior—lever pressing. This period is remembered in a letter I received from Nat in 1973, 28 years later.

Exhibit 9

Actually, you and I had been discussing the possibility of a new introductory course in Columbia College, and a whole follow-up curriculum, in 1943. It all began just after I received my degree in 1942. I had hardly known you up to that time; I had never taken a course with you while a student; I had acquired only the image of your work that . . . C. J. Warden was passing around about you and “that box”; my single semipersonal contact with you was during my dissertation defense, since you were on that committee to quiz me on history. Some months after that . . . I approached you (I remember the scene well) to ask whether I could help in any way with your Morse code work because I wanted to contribute to the war effort . . . and I had been turned down by the armed forces. You let me join you, and . . . in that interaction, you got me (how?) to read the *Behavior of Organisms*. That book produced an explosion in my thinking . . . I talked about it, probing you for clarifications and amplifications and anything I could learn from BFS's work and thought . . . I got a rat and borrowed a “box” from you to see things for myself . . . Soon and somehow the idea germinated between us that there was no reason why a psychology student's education had to start badly; why could we not teach a decent introductory course, and follow-ups, to students as yet unspoiled? Given a base in BFS and reinforcement theory, we didn't see why we couldn't do it. We went on to generate notions like

a course of lectures and extensive lab experience for the student, and like the rat-per-student scheme, the lab sheets and "briefings" and pooling of lab data while still emphasizing the individual organism, the sequence of hopefully little biased experiments with single rats over a whole semester, and so on.

I can remember many details of our conversations and intoxications of those days . . . I remember, too, that the daring of our plans scared us at times . . . And, at one point, you wrote to Volkmann (then at Harvard in some war work) to relate our thinking and dreams for his reaction . . .

The letter to John, asking what he thought about our scheme, produced not just one reaction, but 16, most of which were negative in tenor. The proposal had, he said, attractive features, but we would not get many students in such a course, and our later courses would also have a lower registration. Our political position in the College would suffer, since we'd be serving fewer students than before. The course would be expensive, calling for two instructors, two or more assistants, and a lot of new equipment. Students might be driven away from such a course or poorly prepared for offerings more advanced—Otto's Social Psychology, for example. And so on, but John's 16th point was a simple statement: "The inclusion of Volkmann in the course is greatly appreciated and definitely welcome." This left me with some hope of his participation, which is exactly what we got soon after that.

In the fall of 1946, however, just as we were ready to inaugurate our course, John was offered, and accepted, an associate professorship at Mt. Holyoke College with a large increase in pay. This left me with a heavy burden, until it was decided that Nat should move into the course as his replacement. By the time the class bell rang, he and I, with two assistants, Frederick Frick and Donald Bullock, were prepared to meet our students—or as prepared as we could be.

Nat's memory of our initial laboratory section is almost identical with my own. I quote from him again:

I remember as vividly as anything in my whole career the thrill we both felt on the day the first lab section met to do its first conditioning: the long, meticulous [briefing], the students got their rats [from the vivarium], the darkening of the room, the signal

we gave to all the students to put their levers into the "working boxes" simultaneously, and the suspense of waiting to hear whether any rat would respond—and then the first sounds, and the rising to a full chorus, and our turning to each other to shake hands (while the darkened room hid your tears as it did mine).

To this perhaps I ought to add that we had not had time for testing our equipment or procedure prior to that laboratory meeting. It was indeed a memorable afternoon in Schermerhorn Extension.

Although there were some worries at the outset and some apparatus problems, we got through the year successfully. The student response was very positive and, in the year that followed, we added two more sections (60 students) to our course, and one assistant; the number of our majors increased, rather than fell off; the students who went on to upper-level courses showed no signs of being handicapped; and our science status was enhanced within the College. The weekly meetings of our staff, in which we talked about what had been done the week before and what was planned for the week to come, were lively gatherings in which we all took part, and in which new ideas were presented and pilot studies were suggested. Our lab was busy night and day.

By 1947 there was the smell of smoke on all the floors of Schermerhorn Extension. Two new laboratory courses had been added in the College—one in Discrimination and one in Motivation. Both were taught by Nat and both were natural extensions of our first-course teachings. Two senior seminars were also introduced that year, in areas where we thought our majors might be weakest in when approaching graduate study in other institutions than our own. Murray Sidman and Donald Cook were members of that class who were accepted at Columbia in the year that followed.

In 1947, too, Ralph Hefferline and Fred Frick were given Ph.D.'s for work within our field. Ralph used my laboratory room and Box in an elaborate study of light-avoidance, and Fred did the same for one in visual discrimination. Since Ralph was already a highly-respected member of the College teaching staff, no objections by

Professor Warden were forthcoming and the door was thereby opened to further dissertations with the hated apparatus. Within the years that followed, a steady stream of doctorates were awarded to our protégés, most of whom were sponsored mainly by Nat Schoenfeld. According to my records, nine of these came in the forties, 28 within the fifties, and 21 within the years from 1960 to 1965 inclusive. My list does not include, of course, the names of those who did their bachelor's or master's work with us, but did their Ph.D. research at other institutions—men such as Douglas Anger, James Appel, Sanford Autor, Charles Catania, Hank Davis, Daniel Lilie, Mac Parsons, and Samuel Revusky.

It would be pleasant to report that all these men went into teaching and started little fires wherever they were able, and many of them ultimately did, but more often they had to settle for jobs in psychopharmacology, in military installations, in research institutes, in hospitals, or in other spheres of practical pursuit. Psychology, by and large, was slow to look upon the Box with favor, and I doubt that it will ever really do so.

At the Atlantic City meetings of the Eastern Psychological Association, in 1947, Nat and I, together with Fred Frick and Donald Bullock, reported on our introductory course and our new curriculum. Two years later, this was followed by a more complete account by Nat and me in the *American Psychologist*, wherein we asked our colleagues for their reaction to our program. The reactions that we got were few, and strongly pro or con. Professor Yerkes, at Yale, was very enthusiastic. He called us pioneers and said he hoped that we were setting a pattern that "would help to make psychology a really useful subject for undergraduate instruction."

Frederick Thorne, Editor of the *Journal of Clinical Psychology* and a Columbia Ph.D. (1934), wrote me a four-page letter, complaining about the Department of Psychology in general as "too restrictive in the direction of Behaviorism, experimentalism, and pure science for its own sake." It ignored "the need

for studying the whole individual as a person, of respect for deviant viewpoints such as psychoanalysis."

G. R. Wendt, another Columbia Ph.D. and Professor Warden's former assistant, wrote to the *American Psychologist* complaining about us as a cult of "Skinnerism." "I recognize," he said, "that the Columbia system is enthusiastically received by students, but all cults have that advantage, that simplification introduced into confusion has high acceptance value; . . . Systems have their function when eagerly developed by individuals, but their administrative imposition on a college can only be harmful in the end."

I answered Dr. Thorne in a palliative manner and invited him to visit us, but I ignored Professor Wendt's assault. My friend at Harvard, who had written a reply, since "Skinnerism" was attacked, ended in agreement that no important purpose would be served thereby and decided not to publish. "Local opinion seems to agree with that at Columbia," he said.

Exhibit 10

The psychology section of the Columbia graduate catalogue for 1947 contained the following announcement:

Basic Concepts in Modern Psychology. An introduction to present-day reinforcement theory, with special reference to (1) behavior acquisition and maintenance; (2) discrimination and secondary reinforcement; and (3) punishment, avoidance, and anxiety.

I offered this course for every year from that time on while I was at Columbia. Ten to 20, maybe more, graduate students were usually enrolled. The major textbook was *The B of O*, although, as time went on, other general readings were included. By 1958, the "special reading assignments" in the course included 55 experimental papers. Twenty-eight of them came out of Schermerhorn Extension. (Also in 1958, the *Journal of the Experimental Analysis of Behavior* was established. Its Board of Editors included 13 members, five of whom, including the Editor-in-Chief, were Columbians. A rough calculation tells me that in its first

five years this journal published 260 papers, about 32 per cent of which were contributed by Columbia Ph.D.'s.)

Returning to 1947, I am compelled to admit that at least two other fires got started elsewhere—one at the University of Minnesota and one at Indiana University. There was such a blaze at Indiana that several of us from Columbia went to Bloomington to dance around it with the natives. In my photograph of 19 men on the steps of the psychology building there, I find that eight of them came out of Schermerhorn Extension: David Anderson, Jim Dinsmoor, Fred Frick, Ralph Hefferline, George Klein, Van Lloyd, Nat Schoenfeld, and myself—four graduate students and four teachers. In 1948, there was another powwow, also at Indiana, attended by a larger group; and in 1949 there was another, even larger, at Columbia. There were too many at this meeting for informality and togetherness, so we didn't attempt to hold another.

It isn't possible to take a picture of all the behavior analysts in the world today, but I have a copy of the program for the 1986 Association meeting in Milwaukee. More than 900 men and women from more than 30 different nations were on hand to speak on such a variety of topics as to beggar all description here. The fire that started in the forties at Columbia can no longer be contained.

To the best of my understanding, however, there are no behavior analysts at Columbia University today. The smell of smoke in Schermerhorn Extension has long since disappeared. My retirement year was 1964; Nat Schoenfeld left for Queens College of the New York City system two years later; and Ralph Hefferline's untimely death occurred in 1974. One man who might have carried the torch when Nat and Ralph were gone, was William Wallace Cumming, the youngest member of our staff, but Bill's death preceded Ralph's. The fire was smothered at Columbia for lack of suitable replacements.

Nat Schoenfeld was in some respects the central figure of our group, by virtue of his critical acumen, his efficient working habits, his skill in clarifying complex

matters, his willingness to help a student or a colleague, and his genius in the generation of research. Most of the Columbia contributions to our science in the forties, the fifties, and the sixties have his mark upon them in one way or another.

The Charles Darwin of our group was Ralph Hefferline. I once described him as "a man of science in the classic mold. He was detective of the subtle change. He was an avid searcher for the elusive fact. He didn't know the meaning of expediency or shortcut. He didn't listen to the popular demand or let himself be guided by momentary practical concerns. He was a scholar who never paraded his scholarship; and he was a teacher who reflected these virtues in the classroom . . ." In his doctoral research on light-avoidance, Ralph developed many of his own procedures, spent hundreds of hours in careful observation, and measured thousands of critical responses. At the end, he had several studies, not just one, together with a fresh analysis of the problem and a program of research.

My role in this development was that of a promotor, a kind of missionary, an enthusiastic elder spokesman for the group, attempting to spread the work of reinforcement theory (or whatever we may call it) to my pupils, to my colleagues, and to the world at large.

Nat and Ralph and Bill and I were not the only ones to nurse the fire. There was a series of short-term instructors who worked in the beginners' course while awaiting a better appointment; there were our lab assistants in four courses; and there were students at one level or another of their education. All of us worked together, with little regard for rank or status, with the common purpose of adding to the structure of the system first presented in *The B of O*. Since this system had been outlined only, there was room for contributions from many different quarters.

The Columbia fire had two fundamental sources, a Book and a Box—a system and a research method. To these were added a felicitous combination of people and events. The most significant event occurred, in my opinion, when the De-

partment of Psychology and the College Administration permitted the introduction of an animal-laboratory course for beginning students. We started at the bottom rung of the academic ladder, if I may change the metaphor, after which it was a steady climb until we reached the top—a period of gradual development and expansion, together with some capitulation from those who barred the way.

There is a footnote I must add to this recital. In 1961, I took the Book and the Box to another institution, the University of São Paulo, in Brazil; and, in 1962, J. Gilmour Sherman, a former assistant and colleague at Columbia, replaced me there, adding vital features of the system and the method. We were quartered with the physiologists, in the University City, with a group of fourth-year students of psychology to instruct, together with two young teachers, Carolina Bori and Rodolfo Azzi, who attended all our lectures. We had the full support of the University administration and very little competition of a systematic or experimental nature from the psychologists already there.

Within the period of our two appointments, a primitive laboratory was established, a research project was conducted,

translation of textbooks into Portuguese was started, and three students began their graduate study in the States. Other students followed them and other teachers followed us; other Brazilian universities became involved, and two professional organizations were created there.

In 1985, the Psychological Society of Ribeirao Preto, in the State of São Paulo, had its 15th Annual Renuion, with Murray Sidman as its Honorary President and principal speaker. Also in 1985, the already-established Association for Behavior Modification was reorganized to become the Brazilian Association for Behavior Analysis, which will publish a new journal. This year, at the Milwaukee meeting of our own Association, six Brazilians were slated to deliver papers and one or two others held forth at poster sessions. One of these participants, Maria Amelia Matos, studied with me in 1961 at São Paulo; another was João Claudio Todorov, a pupil of Gil Sherman's in 1962. Both are influential leaders in our field, but they are only two who bear witness to the fact that the little fire discovered at Columbia in 1938 was never fully extinguished. It simply spread to other places.